

FERTILITY OF HYBRIDS FROM THE COMMON AND CHINESE GOOSE

IN the "Origin of Species" I have given the case, on the excellent authority of Mr. Eyton, of hybrids from the common and Chinese goose (*Anser cygnoides*) being quite fertile *inter se*; and this is the most remarkable fact as yet recorded with respect to the fertility of hybrids, for many persons feel sceptical about the hare and the rabbit. I was therefore glad to have the opportunity of repeating the trial, through the kindness of the Rev. Dr. Goodacre, who gave me a brother and sister hybrid from the same hatch. A union between these birds was therefore a shade closer than that made by Mr. Eyton, who coupled a brother and sister from different hatches. As there were tame geese at a neighbouring farm-house, and as my birds were apt to wander, they were confined in a large cage; but we found out after a time that a daily visit to a pond (during which time they were watched) was indispensable for the fertilisation of the eggs. The result was that three birds were hatched from the first set of eggs; two others were fully formed, but did not succeed in breaking through the shell; and the remaining first-laid eggs were unfertilised. From a second lot of eggs two birds were hatched. I should have thought that this small number of only five birds reared alive indicated some degree of infertility in the parents, had not Mr. Eyton reared eight hybrids from one set of eggs. My small success may perhaps be attributed in part to the confinement of the parents and their very close relationship. The five hybrids, grandchildren of the pure parents, were extremely fine birds, and resembled in every detail their hybrid parents. It appeared superfluous to test the fertility of these hybrids with either pure species, as this had been done by Dr. Goodacre; and every possible gradation between them may be commonly seen, according to Mr. Blyth and Capt. Hutton in India, and occasionally in England.

The fact of these two species of geese breeding so freely together is remarkable from their distinctness, which has led some ornithologists to place them in separate genera or sub-genera. The Chinese goose differs conspicuously from the common goose in the knob at the base of the beak, which affects the shape of the skull; in the very long neck with a stripe of dark feathers running down it; in the number of the sacral vertebræ; in the proportions of the sternum;¹ markedly in the voice or "resonant trumpeting," and, according to Mr. Dixon,² in the period of incubation, though this has been denied by others. In the wild state the two species inhabit different regions.³ I am aware that Dr. Goodacre is inclined to believe that *Anser cygnoides* is only a variety of the common goose raised under domestication. He shows that in all the above indicated characters, parallel or almost parallel variations have arisen with other animals under domestication. But it would, I believe, be quite impossible to find so many concurrent and constant points of difference as the above, between any two domesticated varieties of the same species. If these two species are classed as varieties, so might the horse and ass, or the hare and rabbit.

The fertility of the hybrids in the present case probably depends to a limited degree (1) on the reproductive power of all the Anatidæ being very little affected by changed conditions, and (2) on both species having been long domesticated. For the view propounded by Pallas, that domestication tends to eliminate the almost universal sterility of species when intercrossed, becomes the more probable the more we learn about the history and multiple origin of most of our domesticated animals. This view,

¹ Charlesworth's "Mag. of Nat. Hist." vol. iv., new series, 1840, p. 90. T. C. Eyton, "Remarks on the Skeletons of the Common and Chinese Goose."

² "Ornamental and Domestic Poultry," 1848, p. 85.

³ Dr. L. v. Schrenck's "Reisen und Forschungen im Amur-Land," B. i. p. 457.

in so far as it can be trusted, removes a difficulty in the acceptance of the descent-theory, for it shows that mutual sterility is no safe and immutable criterion of specific difference. We have, however, much better evidence on this head, in the fact of two individuals of the same form of heterostyled plants, which belong to the same species as certainly as do two individuals of any species, yielding when crossed fewer seeds than the normal number, and the plants raised from such seeds being, in the case of *Lythrum salicaria*, as sterile as are the most sterile hybrids.

Down, December 15

CHARLES DARWIN

CLOUD CLASSIFICATION¹

THE work of a meteorologist who has devoted himself with great diligence for many years to the study of the structure, forms, and movements of the clouds, possesses a strong claim on the attention of all who are interested in this difficult branch of science. Independently of the importance of the challenge which Prof. Poëy offers to an existing system of nomenclature, his book contains numerous facts and suggestions of very considerable scientific value. In the present enlarged and revised edition the author has endeavoured to satisfy the requirements of our advancing knowledge on the subject of which he treats; a task which ought, unfortunately, to be one of no great difficulty, owing to the small amount of progress which has been made in this, as compared with other departments of meteorology, since the appearance of the second edition.

The history of cloud-nomenclature has been to a great extent a record of wrecks and casualties, because classification has, by an unfortunate necessity, preceded the knowledge of the physical structure of the objects classified. Prof. Poëy was one of the first to appreciate the importance of the fact that the terminology of the clouds must, ultimately, be based not simply upon the varieties of the forms of clouds, but upon those physical conditions to which these varieties are related. But our knowledge of the physical conditions which determine the development of the modifications of cloud is at the present time so limited that no classification founded thereon can as yet be unreservedly adopted. A great deal of questionable hypothesis necessarily enters into the construction of Prof. Poëy's scheme, as he would, we believe, with the candour which distinguishes him, be the first to admit. There is of course a strong *prima facie* desirability that cloud observers should possess some definite system of nomenclature; and at present nearly all of them, not of the lazy class, complain that cloud-classification is still in a state of chaos. Yet it may be doubted whether, for some years to come, a Meteorological Congress will be able to establish an absolutely fixed system of classification which will be universally accepted. Of the ground on which such a system should be built science has hitherto explored but a small portion; and even where we have the materials for observational and experimental research in this direction, very inadequate use has been made of these materials. The immediately practical problem which is raised by the study of this book is this:—In the provisional adaptation of our cloud classification to the status of our knowledge, is it desirable that Prof. Poëy's terminology be adopted in lieu of that of Howard, or should the still prevailing nomenclature be retained, with such modifications as the observations of Poëy and of other students of the subject have as yet shown to be necessary? To this problem we shall venture in the present article to suggest an answer.

As might be expected from the condition of the subject the critical portion of Prof. Poëy's treatise is more successful than the constructive. Several of Howard's terms have had from the first an ill-fated career. To

¹ "Comment on observe les Nuages pour prévoir le Temps." Par André Poëy. Third Edition. (Paris: Gauthier-Villars, 1879.)

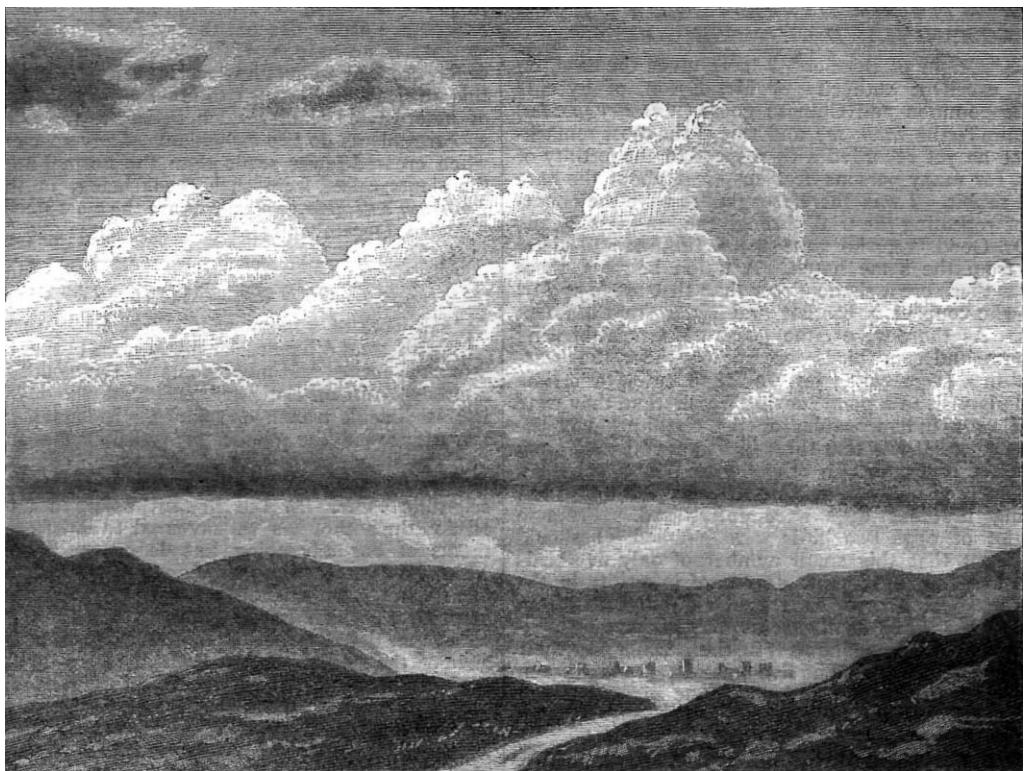


FIG. 1.—“Cumulus” with “Fracto-cumulus.”

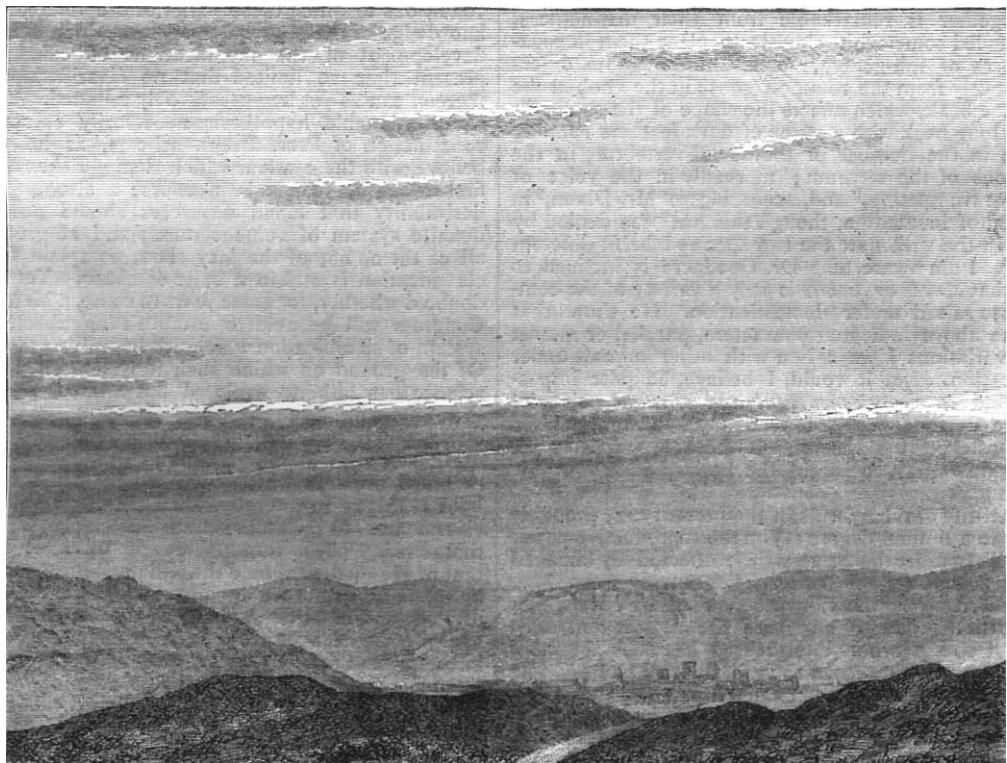


FIG. 2.—“Stratus” with “Fracto-stratus.”

begin with, the name "stratus," which has ever been the object of Poëy's especial animadversion, was unluckily applied by Howard himself to ground fog. The result has been a curious condition of anarchy among the followers of Howard's system up to the present day. The predicament in which these observers have found themselves is this. One of the three primary cloud-names which Howard introduced is never, if his system be rigidly followed, to be applied to any object which most people call a cloud at all. It must be admitted that a fog and a cloud are, in structure, one and the same thing : a cloud is a fog viewed from without, and a fog is a cloud viewed from within. But it is precisely because a fog is, in this sense, a cloud, and not a particular kind of cloud, that it is objectionable in practice to apply to a fog a specific cloud name. An observer may be for hours together

enveloped in a fog of the form of which he can discover nothing, except that the under surface necessarily follows the contour of the earth's surface. In a classification having reference to the shapes of clouds, it is undesirable to give to such a phenomenon a technical name distinctive of a special form of cloud. Prof. Poëy pertinently says "aucun observateur consciencieux ne voudra enregistrer sous le nom de 'stratus' un phénomène de brouillard." On the other hand, a very large class of clouds, occurring in every part of the globe, and in some parts actually the predominant type, have possessed in Howard's terminology no appellation at all, viz., the clouds, neither cumulus nor cirrus, which extend themselves in a bed or layer, whose vertical dimensions as compared with its horizontal are very small. A certain number of observers have freely applied the term "stratus" to this type of cloud. Others,

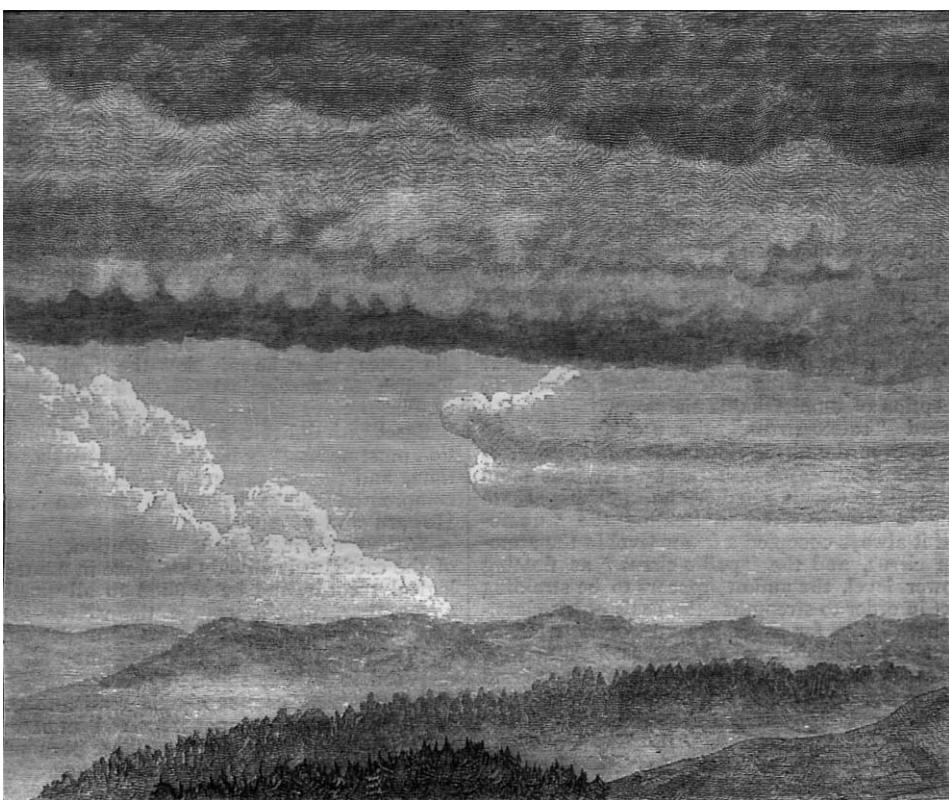


FIG. 3.—"Mammato-cumulus."

perhaps more conservative, have created endless confusion by bestowing the name "cirro-stratus" on all clouds of the description to which we refer, while others again have wrought similar havoc by a corresponding misapplication of another of Howard's compounds "cumulo-stratus." Finally, Prof. Hildebrandsson is driven to the revival of the "strato-cumulus" of Kaemtz, as the title for the prevailing winter-cloud of Northern Europe. Prof. Poëy's remedy for this state of things is to abolish the term "stratus," and to apply to all clouds which lie in beds the title "pallium." The effort has already proved partially successful, for, owing to the defect in Howard's system, "pallium" and its compounds have to some extent replaced, at least among American meteorologists, the "stratus" of Howard and its compounds. Ice-clouds disposed in a sheet or layer are to receive the name "pallio-cirrus"; water-clouds the name "pallio-

cumulus." We think Prof. Poëy's objection to the word "stratus" as applied to a bed or layer of cloud somewhat ill-directed. The term (signifying "levelled," or "laid flat") is in itself quite as expressive as "pallium" (which does not so much involve the idea of the horizontal); and, however it may have been misapplied, we suspect that it will yet prove possessed of too much respectability to be summarily ejected. Prof. Poëy would retain, inconsistently as it appears to us, the compound name "cirro-stratus," but we have always found it extremely difficult to understand precisely what kind of cloud he means to describe under this name, or to recognise with any distinctness what is his idea of "nuage stratifié," the clouds which he terms "stratified" being rather what most persons would call "striated." Whenever cirrus becomes sufficiently extended to form a veil or sheet, it is to receive the name "pallio-cirrus." "Cirro-stratus" is

something which floats at a higher level than "pallio-cirrus." Looking at one of the figures in which "cirro-stratus" is portrayed, we recognise only cirrus, tending slightly to the form cirro-cumulus (Plate 3, Fig. 1). Some of the other representations convey the idea of clouds which would certainly be at a lower level than the usual veil or bed of extensive sheet-cirrus. Some readers may however be more fortunate than we have been in recognising the form of cloud which the author intends to designate as "cirro-stratus." His theory of the formation of the varieties of clouds of the cirrus types is perhaps as clearly expressed in the following as in any passage in the book. "Voici exactement ce qui se passe dans la nature : lorsque les cirrus s'abaissent, ils se transforment en cirro-stratus. Les aiguilles glacées de ce dernier nuage inférieur sont plus compactes et abondantes, plus définies et mieux géométriquement distribuées que les particules moins abondantes et plus isolées des cirrus supérieurs. Quand les cirro-stratus s'abaissent à leur tour, ils se transforment en cirro-cumulus : la structure neigeuse remplace la structure glacée par l'effet de la hausse de la température. Les cirro-cumulus en s'abaissant eux-mêmes, se transforment en pallio-cirrus ou en une couche neigeuse" (p. 29).

The compound "cumulo-stratus" Prof. Poëy would get rid of. We believe that Howard had himself a clear idea of a distinct object when he employed this word; but up to the present time, owing to indistinctness of verbal description, to ill luck as to illustrations, and to other accidents, the word has had a desperately unfortunate history, and Prof. Poëy's objections to it consequently come with great force. It is no exaggeration to say that while we have seldom found two observers really agreed as to the object denoted by this word, we have known the word applied to every existing description of cloud, with the sole exception of unmistakable cirrus.

Poëy's word "tracto-cirrus," the use of which he advocates whenever the cirrus-clouds arrange themselves in parallel bands, is an expressive term. But it is often extremely difficult to decide whether the cirri are or are not arranged in bands. And in treating the "tracto-cirrus" as if it always occupied a lower level in the atmosphere than cirrus, and the "pallio-cirrus" as floating at a still lower level, the author appears to be straining physical fact in order to strengthen the basis of his classification. There is, so far as we are aware, no evidence to show that whenever cirrus adopts, as it does in a majority of cases in most regions of the globe, the band form, it sinks in the atmosphere, and that a further subsidence takes place whenever it spreads itself into a veil or sheet.

The word "nimbus" is to share the fate of stratus and cumulo-stratus. Here again we think the author would have been more successful if he had preferred reform to abolition. That two distinct beds of cloud, the one at a high, and the other at a low level, frequently exist when rain is falling, there is abundant evidence to show, and perhaps this is especially the case during extensive intra-tropical rains. But observers are at least equally agreed as to the fact that a bed of cirrus may coexist with a layer of low cloud, either with or without one or more intermediate layers, without the occurrence of rain or snow. And it is equally certain again that the majority of passing showers are produced in a single mass of cloud, not necessarily, and perhaps never, homogeneous in structure in the portions near the earth and in those which extend into the higher regions of the atmosphere, but certainly not divided into two ocularly distinguishable strata. These facts seem to be ignored by the author when he substitutes "pallium" for "nimbus," and then makes the rainy "pallium" to consist in all cases of "pallio-cirrus" superimposed upon "pallio-cumulus."

As regards the term "pallio-cumulus," we are again obliged to hesitate in accepting it as a thoroughly descrip-

tive title for a layer of low cloud, which has little in common with cumulus except that it occupies much the same level in the atmosphere.

Another of the author's terms, "fracto-cumulus," which he employs for those fragments of low cloud, which though not themselves hemispherical, are nascent or potential cumuli, seems a useful word (perhaps only open to the slight objection that the affix would be liable, if Poëy's system were adopted, to be confounded, especially in MS. reports, with his other affix *tracto*). His French equivalent "nuage venteux" is, however, not sufficiently comprehensive, and is open to the same objection as the English word "scud," which involves the idea of rapid motion. The low cloud-fragments are not necessarily either the concomitants or the precursors of wind.

A highly interesting section is devoted to the clouds which have been in England denominated "pocky clouds." The fact that this name has been applied to several distinct varieties of clouds is certainly not unduly pressed by the author; in truth he scarcely appears adequately to realize the amount of misapprehension which has existed on the subject of these clouds. A Latin affix would certainly have the desirable effect of obliging the observer to give attention to the generic form of cloud from which hang the characteristic bladder-like protuberances; thus clouds of the cirrus and of the cumulus type, presenting this peculiarity, would no longer be registered under a single title. But Poëy's proposed affix "globo" does not appear satisfactory, for there is reason to suspect that some observers would be likely to apply it to any spherical or apparently spherical masses of cloud. How easily, in cloud terminology, misapprehension arises from the mere sound of a name the author himself in this very section illustrates, when he mistakes the "roll-cumulus" of the English Meteorological Office for ill-developed pocky cloud.

Beset with difficulty as all questions of cloud classification must necessarily be, we yet believe that at the present time, and for the present, a useful and unobjectionable compromise might be made between the systems of Howard and of Poëy.

Stratus might, without dissolution, leave the surface of the earth, as it already has done in numerous meteorological records, and be applied to all clouds, not of the cirrus type, which arrange themselves in a horizontal bed. *Cirro-stratus* would then form the descriptive title of the ice-clouds of the higher regions whenever these are disposed in a bed, sheet, or veil. The terms *cumulus* and *cirro-cumulus* may remain undisturbed. If the title *cumulo-stratus* have not received mortal injury from abuse, it might be applied to those peculiar descriptions of "mackerel cloud" or "nuage pommelé," which are only at a moderate elevation, and are not in physical structure cirro-cumuli, a class of clouds which much need a distinctive title. If *nimbus* is to be retained, it might be subdivided into its two essentially distinct varieties, the massive local shower-cloud, and the extensive bank of composite rain-cloud; and for these varieties the titles *cumulo-nimbus* and *strato-nimbus*, or some equivalent expressions, might come into use. The expressive *fracto-cumulus* should have its place secured; and this affix of Poëy's may have further applications; detached pieces of those clouds which tend to arrange themselves in horizontal beds (pieces which are in no sense the nuclei of cumulus clouds), may conveniently be termed *fracto-stratus*, while to the little wavy or broken shreds of ice-cloud which Poëy seems to designate "cirro-strati," the term *fracto-cirrus* might perhaps be applied. For the bizarre "pocky clouds," which, though not nearly so rare as is usually supposed, are certainly not common, an affix, if descriptive, would be none the worse for being somewhat outlandish, and possibly "*mammato*," or even "*papillato*," might be acceptable. If the course suggested in the present article be adopted, no very serious changes will

have to be made in the application of Howard's terminology, and no terms will have either to be coined or to be introduced from other systems of classification into that of Howard, with the exception of the affix "fracto," and the affix "mammato" (or one equivalent to it). We have thought it desirable to give illustrations of the types of cloud to be distinguished by these last names. In the first sketch "cumulus" is shown with "fracto-cumulus"; in the second "stratus" with "fracto-stratus"; in the third the characteristic base of "mammato-cumulus"; and in the fourth that of "mammato-cirrus."

We are not without hopes that Prof. Poëy will be induced to give his aid to proposals of moderate reform in the direction above indicated. We are convinced that he will find it easier to modify, by limitations and expansions, a long existing terminology, wherever the terms

are essentially truthful and expressive, than to sweep it away and introduce another in its place.

To return to the book under notice. "How to classify the Clouds" would be a more descriptive title than that which it possesses. However, the reader who wishes to learn the art of cloud observation, with the view of learning to forecast the weather, will obtain valuable information from the descriptions which the author finds upon his own observations, as well as from those which he quotes from other observers, e.g. the exquisitely truthful description of cirrus quoted from Bravais (pp. 64 and 65). Some of the remarks on the azimuthal rotation of the clouds in Havanna, and on other phenomena, are well worthy of the attention of meteorologists. Here, e.g. is an observation, which, taken in conjunction with the inclination of the axes of cyclones and anti-cyclones in-



FIG. 4.—"Mammato-cirrus."

dicated by cloud-observations in Europe, and also with the recent conclusions of Prof. Loomis as to the sequence of winds at the American mountain observatories, may point to an important general law; "dans le plus grand nombre de cas, le vent anticipe sur les fracto-cumulus, ceux-ci sur les cirro-cumulus, et ces derniers sur les cirrus, c'est-à-dire de bas en haut, au lieu d'être de haut en bas. Ce fait paraît contredire l'hypothèse que les courants supérieurs déterminent, de proche en proche, le passage, sous le même parallèle, des courants inférieurs jusqu'aux vents de surface" (p. 127).

One who writes on a generally neglected subject, to which he has himself devoted much attention, is often tempted to accept too readily as grist anything that comes to his mill, and Prof. Poëy is not altogether free from this tendency, especially in those parts of his works in which he launches out into very questionable hypotheses

both on the theory of winds, and on the action of heat and of electricity upon the clouds. Still more to be regretted is a certain looseness, not so much of language as of conception, which occasionally leads him to make some surprising statements, as well as to employ inaccurate expressions.

He usually speaks of the water-clouds as composed of aqueous vesicles, sometimes of vesicular vapour. In one passage, speaking of frozen clouds, he talks of the "vesicular vapour passing from the state of particles of ice to those of snow" (p. 77).

A protest is necessary against his often repeated definition of cumulus as a "cloud of the horizon." He says (p. 23), "Nous pouvons assurer que, sous toutes les latitudes du globe, les cumulus sont spécifiquement des nuages d'été, de jour, et de l'horizon." And again (p. 104), "Ils demeurent toujours confinés à l'horizon, et ne travers-

ent jamais la région zénithale qu'ils n'atteignent même pas. Cette seule circonstance distingue profondément les cumulus des fracto-cumulus." Truly a profound distinction! We had supposed that in regard to clouds, as in some other matters, "one man's horizon is another man's zenith." Are cloud-classifiers really driven to such extremities? What would be thought of the botanist who appended to his description of the *Ulmaceæ* the statement that "the trees belonging to this family are objects seen upon the horizon," and then proceeded to meet the reasonable objection of the surprised reader by the remark that certainly the elm trees around his (the botanist's) residence, were all seen near the horizon? Yet this is how (p. 24) the author handles his descriptions of cumulus. (The fact of course is that the characteristic form of cumulus is not readily discernible when the cloud is near the zenith.) A somewhat similar remark, made by the author in explanation of the fact that a belt of cirrus, clearly visible towards opposite points of the horizon, is frequently invisible, or nearly so, overhead, a fact of which the optical explanation is obvious, is so strange that we must quote it. "Nous l'attribuons à l'extrême degré de froid que nous avons toujours observé dans la région zénithale, relativement aux autres azimuts. Sous cette basse température et cette extrême sécheresse, la vapeur d'eau se maintient à l'état élastique, et se précipite difficilement sous la forme de filaments extrêmement déliés. C'est pour cela que les cirrus sont plus rares, moins denses et passagers vers la région zénithale," (p. 69).

It is with reluctance that we notice statements of this kind in a work the general idea of which we admire, and in the aim of which we cordially sympathise.

W. CLEMENT LEY

THE PLANETS OF THE SEASON

MARS

IF the two great leaders of the planetary system have filled us with astonishment at their magnitude and velocity, and with perplexity in the contemplation of arrangements so incomprehensibly unlike our own, they have not exhausted all the resources of the season. There yet remains a much nearer and more intelligible neighbour, who possesses a peculiar interest for an opposite reason—his similarity to ourselves. This especial character of the ruddy planet has long been known to astronomers, and will naturally make him an object of careful study before we leave him too far behind; and though the opposition of this year does not diminish his distance so much as about of 1877, yet his almost startling brilliancy has been less though colpove it amongation favourable ones; for English astronomers, at least, it is far more propitious than the last, from his greatly-increased elevation. Much had been expected at that last opposition from the broad expansion of his disk, but the indistinctness of detail was a general source of disappointment here, though the success of Schiaparelli at Milan and Green at Madeira showed that the fault lay chiefly—perhaps not exclusively—in the English sky. My own impression certainly then was that, besides the want of clear outline inseparable from so low an altitude, there was a deficiency in decidedness of form and strength of tone as compared with previous observations, the cause of which may have lain in the atmosphere of the planet, affected possibly by especial proximity to the sun in an orbit of considerable eccentricity. At any rate, we may reasonably hope to find the present season more favourable for exploration than the last; for though at nearest approach we have only had 23" of disk instead of 29" in 1877, success depends, with equal instrumental sharpness, much more upon altitude and steadiness of air than on increase of visible surface. Schiaparelli was enabled

to obtain his most valuable results after opposition, when the diameter had decreased to 20" or even 16", and he asserts that he was able to continue his researches with advantage even till it came down to less than 6".

We have alluded to the special interest of this planet arising from its supposed close correspondence with the earth, and it may not be out of place on this occasion if we bestow a little pains in examining the ground of that supposition. This we may conveniently do by imagining what would be the telescopic aspect of our own globe at a distance not equal to that of Mars, as we should then appear about twice as large, but such as to reduce our apparent diameter to equality with his in a favourable opposition.

There is every reason to believe that our surface would then appear mapped out by a distinct separation into oceans and continents, the fluid being darker than the solid masses, and preserving their bluish-green tinge but little affected by distance. Except in very shallow parts, their darkness would be uniform from the rapid absorption of incident light, and their contour would be sharply defined. The general hue of the land would be lighter; and at a distance where its variegated patches of colour would be separately undistinguishable, the result would be a grey resulting from the mixture of many tints, except where tracts such as the great deserts or prairies might subtend a sufficient angle to preserve their natural hue, or where extensive forests might rival seas in depth of tone. In many places, too, brilliant streaks and patches would show where mountain masses were capped with dense clouds, or surpassed the level of perpetual snow; but our largest rivers, except possibly at some great embouchure, would be totally imperceptible.

Such, in its general lineaments, would be the distant aspect of our globe, if the whole lay at once distinctly before the eye. But this would never be the case. The formation and transference of masses of vapour would produce incessant and most uncertain changes. In some regions and at certain times of year there would be unbroken clearness; in other tracts the outlines and colouring of land and sea would be indistinct, or concealed, at times for short, but occasionally for very lengthened, periods. And the interposition would doubtless be always of a white aspect, since such is the character of our clouds wherever they are illuminated by the sun. Towards our polar regions this whiteness would be permanent in the form of great spots, excentric as regards the axis of rotation, increasing through and after the winter, with a corresponding diminution after the summer solstice. There would always be, however, a large unmelted area, even at the warmest period, and its outlines would probably be on the icegale and extended from the presence of great features of fregns clouds. Now, it chese would be the probable features of the earth, presented to us at a distance of seventy or eighty millions of miles, in what respects shall we be able to trace the resemblance on Mars? We are soon brought to the conclusion that, according to the general rule already referred to, there is more analogical than identical correspondence: the inclination of axis, the excentricity of orbit, the duration of day and night, the respective length of the seasons—from the relative similarity but not identity in these particulars, we are prepared to meet with the same kind of proportion throughout. As far as aspect goes, a solid and fluid condition may be thought to divide each superficies; but if so, the land there is in a much larger ratio to the water; and if the colour of our oceans is repeated on Mars, we have little to correspond with the orange-yellow tinge which, since it leaves unaffected the polar snows, cannot arise from atmospheric absorption. The so-called seas, too, though in some places apparently deep and dark, frequently shoal off and show subaqueous markings in a way that perhaps would be scarcely paralleled in our own.